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"Hindcasting the Continuum of Dansgaard-Oeschger variability: Mechanisms, Patterns and Timing"
[Paper CP-2013-120] to Climate of the Past
Response to reviewers

We thank Prof. Maria Fernanda Sánchez-Goñi and an anonymous Reviewer for their constructive comments, which helped to improve the manuscript.
Please find below a detailed point by point response (blue).

Interactive comment on "Hindcasting the continuum of Dansgaard–Oeschger variability: mechanisms, patterns and timing" by L. Menviel et al.
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General comments
The manuscript submitted to The Climate of the Past Discussions by Menviel et al. presents new LOVECLIM simulations for Dansgaard-Oeschger (D-O) and Heinrich events (HE). These simulations are designed in such a way that freshwater forcing in the North Atlantic Ocean generates changes in the Atlantic Meridional Overturning Circulation (AMOC) that produce realistic sea surface temperature changes (SST) when compared with alkenone-derived SST in the Iberian margin. The novelty of this contribution lies on the fact that these simulations are constrained by local observations. Despite this weak constrain, the comparison between independent observations and the simulated climatic conditions (for example, the variability of temperature and hydro-climate in the eastern Mediterranean region, AMOC changes, Greenland temperature changes) indicate a strong agreement. The authors conclude that this high level of agreement provides strong support for their initial hypothesis: "Heinrich events and DO variability during MIS 3 were caused by Northern Hemisphere ice sheet calving and freshwater discharges which subsequently influenced the strength of the AMOC, poleward heat transport and eventually global climate". This work deserves publication in The Climate of the Past. However, I have a number of concerns that the authors should address prior the manuscript be accepted by CP.

In the Introduction, it would be worth that the authors cite the main hypotheses put forward to explain D-O climatic variability and Heinrich events.
We rephrased the introduction to be more explicit on the previous hypotheses put forward to explain DO cycles: "The origin of this prominent variability still remains elusive with proposed mechanisms ranging from internal ocean-sea-ice climate instabilities (Timmermann et al. 2003,

Dokken et al. 2013), to coupled synchronized ocean-ice sheet variability (Schulz et al. 2002), North Atlantic sea ice (Li et al. 2005, Li et al. 2010) or sea ice-ice-shelf fluctuations (Petersen et al. 2013) and externally solar-driven reorganizations of the ocean circulation (Braun et al. 2008). "

Besides that, the key question (iii) "What sets the length of these events and their 'periodicity'?" should be rephrased. I do not understand if the authors address the length of the change itself or the length of the climatic phase produced by the change, I mean D-O warming/cooling versus Greenland Interstadial (GI)/Greenland Stadial (GS) and HE versus Heinrich Stadial (HS) and Heinrich layers (HL).
We took out the key questions that were in the Introduction and instead we emphasize on trying to show that "Heinrich and DO stadials are part of a continuum of variability that is generated through ice sheet/AMOC interactions."

The terms used by Menviel et al., are confusing and the reader cannot easily follow author's arguments. HE are different from HS and HL (see discussion in Sanchez Goñi and Harrison, 2010, QSR).
We understand the different terminology and try now to use the appropriate terms (HE, HL and HS) as needed.

The titles of sections 3.5 and 3.7, "Abruptness of events" and "Timing and duration of events", respectively are now misleading because the authors discuss in 3.5 the duration of the event (D-O warming, i.e. a change) and in 3.7, as in Table 1, the duration of the climatic phase triggered by HE (recorded in the sediments by the presence of IRD that we call HL), i.e. HS. This part of the manuscript needs to be reorganized and clarified.
Section 3.5 was renamed Abruptness of stadial/interstadial transitions
and section 3.7 was renamed "Timing and duration of Heinrich stadials"

In Table 1, it is really surprising to see that the authors discuss the duration of the HS based on terrestrial and ice paleoclimatic archives where there is no any direct tracer for HS, i.e. Ice Rafted Detritus (IRD). I suggest to the authors to look at the duration of HS based on the compilation of North Atlantic sequences by Elliot et al. (1998, 2001) (see discussion in Sanchez Goñi & Harrison, QSR, 2010).
Based on section 3.5. as well as on previous work done on Heinrich stadials, we assume that Heinrich stadials will have a similar timing and duration in the North Atlantic and elsewhere. We are using speleothems records because they have an independent and relatively robust chronology.
"We will focus in particular on Heinrich stadials (HS5, HS4 and HS3) and stadial C7 (see Figure1) and use high resolution paleoclimate reconstructions with independent age control that capture Heinrich and DO variability."
We also now compare our estimate with the estimate of Sánchez Goñi & Harrison, QSR, 2010.
"These ranges also agree well with estimates from North Atlantic marine sediment cores for the timing of HS5 and HS4 (respectively 50-47 ka B.P. and 40.2-38.3 ka B.P.) (Sanchez Goñi & Harrison, 2010).

I am a little bit worried about the proxies used to perform the comparison between simulated and observed climatic variables. In particular, 18O and L* are not direct tracers for quantitative temperature and precipitations changes. I suggest to the authors to compare their simulations with pollen-derived quantitative temperature and precipitations estimations from the western Mediterranean region (Sanchez Goñi et al., 2002 Climate Dynamics).
We added in section 3.4: "It should be noted that reflectance and magnetic susceptibility are indirect hydroclimate proxies and thus cannot give quantitative estimates. In addition, speleothem $\delta^{18}O$ can be potentially affected by other processes such as changes in temperature, soil evaporation and the water vapor sources."

We now also include a comparison between simulated precipitation over the Iberian region compared to a composite of pollen-derived precipitation estimates from the Iberian margin and the Alboran Sea (Figure 5) (Sanchez Goñi et al., 2002 Climate Dynamics).

The authors say that it is demonstrated that rainfall changes in the Arabian Sea track millennial-scale SST variations in the North Atlantic region. However, the age model uncertainties of North Atlantic marine records and the Arabian Sea record preclude demonstrating such a synchrony.
We added in section 3.4.:
"Age model uncertainties associated with the Arabian Sea record (Deplazes et al. 2013) could preclude any conclusions with respect to synchronicity with North Atlantic stadials. However, the high level of correspondence between our simulated precipitation changes and the Arabian Sea reflectance record indicate that North Atlantic stadials lead to drier synchronous conditions over the Arabian Sea."

In the Conclusion section, the authors conclude that Heinrich events and D-O variability during MIS 3 were caused by Northern Hemisphere ice sheet calving and freshwater discharges based on the strong agreement between simulated and observed paleoclimatic reconstructions. I think that this assertion is a circular reasoning because they have caused climatic changes by introducing freshwater fluxes into the model. Actually, what the authors demonstrate is that LOVECLIM is able to realistically simulate observed climatic changes in different regions of the Earth when it is forced by a particular amount of freshwater fluxes, constrained by local observations (SST in the Iberian margin), and a source located between 55W-10W, 50N-65N. Menviel et al. work's demonstrate that these particular freshwater fluxes explain the observed regional millennial-scale climatic variability of MIS 3. However, the question of the origin of freshwater fluxes remains: What is the cause of the iceberg pulses?
We think our modelling experiment can show that the DO variability observed in proxy records can be explained by changes in the AMOC. Heinrich events and DO cycles would thus be part of a continuum of variability, in which changes in the oceanic circulation plays a significant role. We thus force the model by adding freshwater in the North Atlantic so that we obtain either a 50% weakening or an almost complete shutdown of the AMOC during MIS3. There is no circular argument there, as changes in the AMOC could lead to climate variations at odds with the proxy records. Now it is true that we cannot give a definite answer as to why the AMOC varied.
However, the occurrence of IRD peaks contemporaneous with decreases in North Atlantic SST seem to indicate that iceberg melting also played a significant role in weakening the AMOC.
We agree with previous studies suggesting that a weakened AMOC can feedback onto the ice-sheet and induce iceberg surges.

Also in the conclusion, the paragraph dealing with Figure 11 is confusing. First, the colors of IRD and salinity curves in the figure do not coincide with those referred in the text. There is no salinity axis, and there is no record of AMOC changes to argue that AMOC strengthening leads to North Atlantic and Greenland warming as well northern North Atlantic sea-ice retreat. Actually, there is no record for sea-ice changes. Further, I wonder whether this paragraph is well placed in the conclusion section. I recommend to the authors to rewrite the Conclusion section.
A salinity axis was added to figure 11 as well as the North Atlantic ARM data (Kissel et al. 2008) as a proxy for AMOC changes and summer SST estimates from the Irminger Sea (Van Kreveld et al. 2000). The colors were corrected in the text. We think this figure and the associated text summarize well a possible sequence of events surrounding Heinrich event 4 and is therefore appropriate for the conclusion.

Specific comments
Introduction
The duration of MIS 3 is slightly different, 59.4-27.8 ka, from that presented by Menviel

et al., 60-24 ka. (see discussion in Sanchez Goñi and Harrison, QSR 2010).
The length of MIS3 was modified to 59.4-27.8 ka B.P.

When referring to the large-scale drying of the northern tropics in response to HE, authors should add that there is actually a large-scale drying of the northern Eurasia as shown by the global compilation of pollen data (Harrison and Sanchez Goñi, 2010).
In the Introduction, we added the reference to a large-scale drying of Eurasia shown in Harrison and Sanchez Goñi, 2010.

Sections 3.3 and 3.4
Could the authors specify the direction of the changes in benthic 18O associated with the IRD pulse in the Greenland and Irminger Sea. Are these changes during C7 observed in the twin core MD95-2042 (cf. Shackleton et al., 2000)?
In marine sediment cores from the Irminger basin (SU 90-24 and SO 82-5) (Elliot et al. 2002, Van Kreveld et al. 2000), benthic $\delta^{18}O$ increases during H events by about 0.3 permil. In a series of marine sediment cores from the Rockall Plateau, benthic $\delta^{18}O$ decreases during H1, H3, H4, H5 and H6 (Sarnthein et al. 2000). Finally in a marine sediment core from the Iberian margin (MD 95-2042) (Shackleton et al. 2000), benthic $\delta^{18}O$ decreases by 0.3 permil during H5, H4 and C7. Benthic $\delta^{18}O$ is influenced by a number of processes namely input of isotopically light $\delta^{18}O$ from iceberg discharge, changes in sea-ice, temperature and brine water spikes. Therefore, at this point, we rather not discuss changes in benthic $\delta^{18}O$.

The authors have obtained a comprehensive spatial view of the simulated D-O warming from an EOF analysis of global surface air temperatures and precipitations. It may be interesting to compare these maps with the pollen-derived temperature and precipitation reconstruction at D-O warming 8 (38 ka) and 6 (33 ka) (Harrison and Sanchez Goñi, 2010, QSR).
We added in Section 3.3.: “The warming extends into North Africa, Asia and the western North Pacific. This warming pattern is in general agreement with pollen-derived temperature reconstructions for GI8 (38 ka B.P.) and GI6 (33 ka B.P.) (Harrison and Sanchez-Goni 2010).”

In section 3.4.:
“Comparing the simulated Northern Hemisphere rainfall changes on a regional scale with hydroclimate reconstructions for the Mediterranean region (Sanchez Goni et al. 2002) (Figure 4b), the Cariaco Basin (Deplazes et al. 2013), the Arabian Sea (Deplazes et al. 2013), Eurasia (Harrison and Sanchez Goni 2010), eastern China (Wang et al. 2001), (Figure 5) and Central America (Hodell et al. 2008) (Figure 6, upper panel) we find an excellent agreement between model and data with stadial (interstadial) conditions corresponding to increased aridity (pluvials).”

Section 3.7
The authors should refer to Grousset et al. (2000) regarding the HE 3. These authors conclude that HE 3, in contrast with the other HE, has a European origin.
We now added in section 3.7.:
“Paleorecords as well as the model results display little coherency regarding the amplitude and the timing of HS3. It was suggested (Elliot et al. 1998, Snoeckx et al. 1999, Grousset et al. 2000) that contrary to other Heinrich events, HS3 may have originated from the Fennoscandian ice sheet. It is thus possible that iceberg discharges during Heinrich event 3 had a different impact on the AMOC than for other Heinrich events.”

Figures and Tables
In the legend of Figure 1 and in the Conclusion section, the authors refer to Alley (2000, PNAS) to illustrate Greenland quantitative temperature changes over MIS 3. However, Alley’s paper only presents Greenland temperature changes between 16,000 and 10,000 years. I guess that the

temperature profile presented in this figure derives from estimations by Huber et al, EPSL, 2006.
The manuscript by Alley et al. 2000 indeed shows only the deglaciation, but the data provided on the ncdc website with its suggested reference ranges from 49 ka B.P. To 0 ka B.P.
ftp://ftp.ncdc.noaa.gov/pub/data/paleo/icecore/greenland/summit/gisp2/isotopes/gisp2_temp_accum_alley2000.txt

In the same legend of Figure 1 and all over the text and figures, the authors should replace GIS with GI after Svensson et al., 2006, QSR.
We replaced GIS by GI.

In the legend of Figure 3, what does NE mean?
We replaced NE by northeastern.

Typo mistakes
Page 4773, paragraph 20 – Add "of" in "causing a weakening of the Atlantic Meridional Overturning Circulation).
Corrected

Page 4773, paragraph 25 – Replace "a" with "an "in "evidence of aninterhemispheric".
Corrected

Page 4774, paragraph 5 – Delete "is" in"does not provide is an explanation"
Rephrased

Additional references to discuss and include in the manuscript
Grousset, F. E., Pujol, C., Labeyrie, L., Auffret, G., and Boelaert, A. (2000). Were the North Atlantic Heinrich events triggered by the behavior of the European ice sheets? Geology 28, 123–126.
Harrison, S.P., Sanchez Goñi, M.F. (2010) Global patterns of vegetation response to millennial-scale variability and rapid climate change during the last glacial period. Quaternary Science Reviews 29: 2957-2980.
Shackleton, N.J., Hall, I., Vincent, E. (2000). Phase relationships between millennial-scale events 64,000-24,000 years ago. Paleoceanography 15, 565-569.
Sanchez Goñi, M.F., Cacho, I., Turon, J.-L., Guiot, J., Sierro, F.J. , Peypouquet, J.-P. , Grimalt, J. & Shackleton, N.J. (2002). Synchroneity between marine and terrestrial responses to millennial scale climatic variability during the last glacial period in the Mediterranean region. Climate Dynamics 19: 95-105
Sanchez Goñi, M.F., Harrison, S.P. (2010) Millennial-scale climate variability and vegetation changes during the last glacial: concepts and terminology. Quaternary Science Reviews 29: 2823-2827.

Interactive comment on "Hindcasting the continuum of Dansgaard–Oeschger variability: mechanisms, patterns and timing" by L. Menviel et al.
Anonymous Referee #2
Received and published: 20 October 2013

The paper provides new information about dynamics underlying millennial scale variability using transient global simulation with an intermediate complexity model for the period 50 ka to 30 ka BP. The main results of the paper are two fold: to assess the validity of this kind of model to hindcast climate changes with glacial boundary conditions, and therefore to examine the different response characteristics of various climate variable at regional scale to AMOC changes based on a close comparison with paleodata. A schematic view is tentatively proposed to explain the effect of the Northern Hemisphere ice sheet instabilities on global climate at millennial time scale. In general the paper is well written and contain all elements for a relevant discussion. To my opinion this study constitutes a very interesting approach and is suitable for publication in Climate of the Past.

However the Take Home Message of the paper should be strengthened, in particular some of the questions listed in the introduction are not clearly addressed in the discussion part. Taken together, parts of the introduction and discussion need to be clarified. This concerns two major points and minor points that need to be taken into account before publication and that I develop below.

-Point 1: Climate mechanisms This is an important issue of the paper that could be more emphasized, in particular in the Introduction part. In the present version, three "key" questions are listed in the Introduction part, for which (at least for the last 2 questions) I do not see any clear answer in the discussion (presented as a long conclusion).
We rephrased the introduction and we took out the key questions that were in the Introduction and instead emphasize on trying to show that "Heinrich and DO stadials are part of a continuum of variability that is generated through ice sheet/AMOC interactions."

To clarify the message of the paper I would suggest to revise the important issues of the paper in order to provide a more balanced discussion to highlight the ability of the model to capture the dominant modes of HE and DO variability and the overall good data/model agreement. Following this point, part of the discussion (rather than in the conclusion part) should be devoted to a more detailed discussion of the dynamical processes based on the physics of the model and the data/model comparison; for example, what causes -enhanced tropical wind during HE/stadials, - a decrease in Asian monsoon activity during HE, -the muted temperature response in central Europe compared to the western Mediterranean Sea. To my opinion the discussion needs also to mention processes that are not fully captured by the models and that still remain unsolved: for example, results in Naafs et al., (2013) paper report a warming in the North Atlantic during HE, involving a northward expansion of the subtropical gyre.
We added a concluding statement on the main mechanisms at play:
"A marked weakening of the AMOC reduces the oceanic and atmospheric poleward heat transport thus leading to a strong cooling of the North Atlantic region. In our model the cooling is centered on Scandinavia, extends over Greenland and Northern Europe and is also simulated over Southern Europe, North Africa and Asia. The cooling is the strongest at high latitudes due to sea-ice albedo feedbacks. Such temperature changes lead to a stronger temperature gradient over the North Atlantic and therefore to a strengthening of the North Easterly trades. The cooler conditions over the North Atlantic and stronger trades induce a southward shift of the ITCZ over the Atlantic region with drier conditions simulated over Europe, the north part of South America, North Africa and the Middle East."

The last point related to climate mechanisms concerns the freshwater forcing which has been prescribed in order to obtain a match between modelled and observed SST on the Iberic margin. A comment is missing about the magnitude of the freswater flux used in this study compared to other studies simulating HE or DO type events.

Section 3.1 describes the freshwater forcing and compares it with proxy records as well as the study of Jackson et al. 2010. The freshwater forcing applied in this study varies between 0.05 and 0.2 Sv. This forcing is comparable to the freshwater forcing used in other studies (See Kageyama et al. 2012 CP for example).

-Point 2: Model / data comparison One of the strength of the paper is to show some comparison between modelled results and paleo-data (SST, d18O, SSS, reflectance, .). While the modelled temperature and precipitation captures very well temperature and precipitation proxy variability, it should be mentioned that some of the proxy are not direct indicators of these parameters particularly for d18O which can be controlled by remote signals rather than local (water vapor source for example, see Legrande et al., 2010).

We added in section 3.4: “It should be noted that reflectance and magnetic susceptibility are indirect hydroclimate proxies and thus cannot give quantitative estimates. In addition, speleothem $\delta^{18}O$ can be potentially affected by other processes such as changes in temperature, soil evaporation and the water vapor sources.”

We now also include a comparison between simulated precipitation over the Iberian region compared to a composite of pollen-derived precipitation estimates from the Iberian margin and the Alboran Sea (Figure 5) (Sanchez Goñi et al., 2002 Climate Dynamics).

In the second part of the conclusion, SSS reconstructions in the Nordic seas are considered as representative of the salinity of the North Atlantic. I would introduce this SSS reconstruction more carefully since the Nordic Sea (Irminger basin) reveal a highly variable environment also shown in Elliott et al., (1999). While the Irminger basin reveals a synchronous response with the central north Atlantic during Heinrich events, it is highly influenced by coastal ice sheet and ice shelves from the Nordic area at millennial scale.

Surface salinity changes in the Nordic Sea will also influence the strength of the AMOC and therefore are relevant for the discussion on AMOC changes during MIS3. To be more specific in the text, when discussing salinity changes, we replaced “North Atlantic” by “Nordic Sea”.

I am not sure if introducing the term “Heinrich event 3.2” is really relevant for this paper. This cold event is well expressed in SST reconstructions but does not appear very clearly in North Atlantic IRD records. Therefore a more detailed discussion should be necessary to look at the spatial distribution of this event over the whole North Atlantic, but as already said it does not add value to the paper.

The composite of core SO82-5 and PS2644 shows that a pronounced IRD peak is associated with HS3.2. It is also seen in another core from the Irminger basin (SU90-4) and from the Rockall Plateau (NA 87-22) (Elliot et al. 2002). In addition, as seen in our paper, H3.2 is associated with strong climatic anomalies around the North Atlantic basin and elsewhere. We therefore think it is interesting to highlight it so that further paleoclimate records can try to identify this event more clearly.

-Minor points

-Concerning the EOF analysis results, I was wondering if it is worth to discuss for precipitation the first EOF, which explains 16% of the variance?

This is very typical for rainfall EOFs to have a low explained variance for the 1st mode. Rainfall is so noisy that the noise gets distributed over many other EOFs. So typically the first mode gets only 15-30% of the global variance. The local explained variance in the tropics is of course much higher.

-Is it possible to merge fig. 2 and 3 since same parameters appear (Med. SST) ?
We considered this option however we prefer to show figures 2 and 3 separately as figure 2 shows the “forcing”: the freshwater input as well as the targeted AMOC and Iberian SST changes. In contrast, figure 3 shows some modeling results compared to paleoproxy records that were not used to constrain the model.

-The discussion in the section “Timing and duration of events” reveals differences between HE timing and timing of Heinrich layers in the sediments of about 3ky. Could you shortly comment on this point?
We actually deleted this sentence as we think that the discrepancy in timing is mostly due to the conversion of 14C age to calibrated ages. As can be seen for the new INTCAL09 calibration, 35 ka in 14C age is equivalent to ~40 ka B.P (Reimer et al. 2009). In addition some cores were tuned to the GISP2 records which shows some 2k discrepancies with the new GICC05 record (Obrochta et al. 2013).

-Fig. 10 is not very clear: why is there a one way arrow for temperature and precipitation boxes? In the discussion related to figure 10, it is stated that “instabilities from the Laurentide ice sheet were associated with much larger iceberg” leading to the HE. Say it more carefully since HE3 is mainly related to the Fennoscandian ice sheets (see Grousset et al.).
We put one way arrows for temperature and precipitation as we think these values do not feedback positively onto the AMOC during H events. However they are a direct result of AMOC changes. Grousset et al. 2001, suggested that in contrast to other Heinrich events, H3 did not have a Laurentide origin. In addition, the climatic impact of H3 seems to be of reduced amplitude compared to the other Heinrich events.

-Why is the considered time interval changing in figure 11?
In figure 11 and the associated text, we suggest a sequence of events to explain the DO variability. For illustrative purposes we decided to focus on the period 40 to 34 ka B.P.